

slight inaccuracies of thought and language occur. Is it really the case, for example, that rate of interest (p. 181) is totally independent of time?

LETTERS TO THE EDITOR

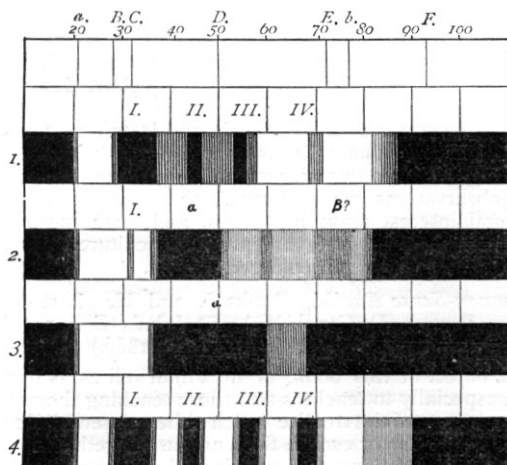
[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Chlorophyll

IN a short note in NATURE (vol. xxxii. p. 342) I mentioned the discovery of a colourless substance produced by the action of reducing agents on chlorophyll and regenerating, on being exposed to the air, the original green solution. I may be allowed to add to my previous statement that if this reaction is not pushed too far and the resulting substance is duly concentrated, it is not colourless, but of a reddish-brown by daylight, of a splendid ruby red (very different from the well-known port-wine red colour of chlorophyll) by limelight. Its spectrum is chiefly characterised by the total absence of band I., and the presence of a broad band corresponding exactly to band II., and the two intervals between I. and II., and between II. and III. Band IV. seems also to be present, though somewhat altered in its position and intensity.

The presence of a slightest trace of oxygen is immediately announced by the appearance of the I. chlorophyll band, so



that the reaction may be considered as a most sensitive test for oxygen. On further exposure to the air, as already mentioned, chlorophyll is regenerated. This new substance being evidently a product of reduction of *chlorophylline*, the green-colouring matter of chlorophyll isolated and described by me in 1869, it may be called *protochlorophylline*, or simply *protochlorophylline*.

Its solutions can be kept in sealed glass tubes containing H_2 or CO_2 : in this latter case in a dark place, for on being exposed to light they turn green. Can it be inferred from these facts that the oxidation takes place at the expense of CO_2 —that carbonic acid is actually reduced under the joint action of light and of a chlorophyll solution? The question, if answered in the affirmative, is of so great importance, that I am now taking all the pains to arrive at a definite conclusion.

The optical properties of *protochlorophylline* seem to indicate its presence in freshly-prepared chlorophyll solutions. Indeed the difference presented by the spectrum of a freshly-prepared green solution and that of Mr. Stokes's *modified chlorophyll* may be easily accounted for by the presence in the former of the broad *protochlorophylline* band intercepting the rays of light in the two intervals between the bands I. II. and III., as just mentioned. To the presence of different quantities of *protochlorophylline* may be likewise attributed the varying relative intensity of the bands II. III. IV.,—a fact that has attracted the attention of many observers.

At all events, it cannot be doubted that the study of this curious substance, though attended with considerable difficulties, all the operations taking place in a total absence of oxygen, and under the continual control of the spectroscope, will throw a new light on that most important of physiological problems—the part played by chlorophyll in the decomposition of carbonic acid by the living plant.

C. TIMIRIAZEFF
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The Stone Age in the Malay Peninsula

IN NATURE, vol. xxxiii. p. 377, there is a notice of a paper by M. de Morgan, published in *Cosmos*, on the Stone Age in the Malay Peninsula. Will you permit me to offer a few remarks with reference to this matter. In the first place, it is said that M. de Morgan came into contact with three native races, which he respectively names Sakayes [Sakai], Seumangs [Sémang], and Rayats [Ryot]. I have put in brackets the commonly-accepted spelling in the Straits. It is funny what peculiar mistakes travellers make when passing through a country the language of which they do not understand, ryot being the word used in the Straits to express those followers or retainers of a native chief who are not actually his debt slaves, but who owe him more or less of feudal allegiance; Malays here invariably use the word when speaking of the following of a Sakai chief. The word ryot is, I believe, also used in the same sense in India. With reference to the tribes of whom M. de Morgan speaks as living in the recesses of the mountains, and whom the Sakaies called "fire apes," I cannot help remarking that I have never heard the Sakaies speak of them myself, nor can I find that any other Government servant here has heard of them either; still we are in pretty constant communication with certain of the Sakaies of these hills, and for my part I have at different times stayed for longer and shorter periods at the clearings of some of the chiefs whom M. de Morgan visited, and moreover I have employed most of the same Malays who followed M. de Morgan. By the bye, these were Sumatran Malays, and they told me some very extraordinary tales about the wild tribes before I started up country with them; these foreign Malays are especially addicted to telling marvellous tales of the wild tribes of the mountains, but so far I have not been able to verify their information in the least degree either from the Sakaies themselves or from native Malay sources. It would be interesting to know what equivalent was used for the expression "fire apes." Was it a Malay word or a Sakai word? With reference to the Stone Age I quite agree with M. de Morgan in believing that at a not very late period—probably just before the Malay invasion—there were tribes living in the interior who were not acquainted with the use of iron; up to the present moment I have been able to collect twenty-two stone implements. I have sent drawings and notices of these to the Anthropological Institute. I may, however, here mention that of these twenty-two specimens one is the half of a stone bracelet; the rest are all chopping-tools of different descriptions, used, I think we may fairly conclude, by a race of boat-builders, who most likely constructed dug-outs, much like the Malays of the present day. I adduce this supposition from the fact that of my twenty-one specimens two are perfect gouges, and six others are of the description which Dr. Evans has classed under adzes. The cutting-edges of nearly all my specimens have been considerably damaged by use. The high polish which M. de Morgan's specimens—and mine also—exhibit is, I think, accounted for in a great measure by the fact that they are used and very highly prized by the Malays as whetstones; the women preserve them, especially to sharpen their razors on, with which they shave the heads of their children during the periods ordered by custom or religious law; and the men were, until lately, very anxious to procure them to sharpen the iron spurs used in cock-fighting. As almost all of the specimens procured by me have been purchased of Malays who have inherited them from their ancestors, and prized them as heir-looms, it is, I think, reasonable to suppose that in their original condition some of them, at least, were considerably rougher than when they came into our hands; this supposition is further confirmed by a remark made to me the other day by a Malay chief. He said that he once had a thunderstone given to him which was so rough that he had to wear it down on his emery-wheel before he could use it as a whetstone. I have one specimen which has no cutting-edge, but is squared off at each end and is almost spindle-shaped. I have

also seen another specimen of the same description. Can it be that these two specimens have been manufactured by Malays for whetstones out of the so-called thunder-stones? I cannot account for them in any other way; they are too slight for hammers.

For my part, I have always found the Sakaies especially wanting in every respect as to traditions beyond the memory of their own generation, and they have invariably answered my inquiries as to the origin of the stone axes by saying, like the Malays, that they are thunder-stones. To such an extent is this belief held by Malays, that the other day a Malay of considerable social standing assured me that once a cocoa-nut palm was struck by lightning close to his house, and that about a month afterwards he searched about the roots of the tree and found the thunder-stone which was the cause of the damage: it was this man's father who for several years kept a fire alight in his house, which fire was generated from the same tree after it had been struck by the electric current.

I have already drawn attention to the Malay belief as to these so-called thunder-stones (*NATURE*, vol. xxxii. p. 626). My specimens are all in the Perak Museum at Thaiping.

Kinta Perak, April 5

A. HALE

On a Thermo-electrical Phenomenon in Connection with Prof. Balfour Stewart's Paper on Terrestrial Magnetism

IN the *Philosophical Magazine* for May Prof. Balfour Stewart, in his paper "On the Causes of the Solar-Diurnal Variation of Terrestrial Magnetism," takes in one place (p. 443), for an example, the case of "an ordinary electric circuit, say of a circular shape, and horizontal, and heat it by causing some source of heat, such as a lamp, to travel slowly around it with a definite rate of progress." He goes on to say that no current due to the heating will take place. So it would generally be thought. If, however, the experiment be even roughly tried, at all events with an iron or nickel wire, the contrary takes place. An account of the experiments, &c., which I have made on this subject, was read before the Royal Dublin Society on March 24, and will, in the course of time, be printed in the Society's *Proceedings*. Though there is a current in a wire on causing a heated portion to travel along it, it seems unlikely from the nature of the phenomenon that it could in any way be inferred that the higher air would similarly affect a current under the sun's heating.

FRED. S. TROUTON

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Do Migratory Birds Return to their Old Haunts?

MUCH evidence has been given by naturalists to prove that birds of passage return to their old haunts. The following, I think, may be of interest to some of the readers of your valuable paper. For the past two springs a cuckoo gifted with a decidedly peculiar note has visited this neighbourhood. Within the last fortnight it has again arrived. Its song consists of three clear distinct notes, cuck—coo—coo, the second note being a semitone above the last. This it never varies. We all know that towards the end of its sojourn the cuckoo suffers from hoarseness, or, as the country people say, "changes its tune." Although this bird suffers in a similar way, yet it still maintains its peculiar song—three notes. As far as I can ascertain, it does not wander beyond the same limits—from the park here to a little hill about half a mile distant. I think these facts not only conclusively prove that the cuckoo returns to its old quarters year after year, but that it also restricts itself during its stay in this country to the same locality.

F. C. TAYLOR

Summerleaze, East Harptree, May 13

The Poison of the Stinging-Nettle

IN the interesting article in your issue for May 6 (p. 5), on "Plants and their Defences," there is one sentence on which I should like to be allowed to offer a few remarks. It runs thus:—"This fluid [of the stinging-gland of the stinging-nettle] is generally conjectured to be formic acid—a view based on the fact that this acid can be obtained from the nettle-plant by suitable means." Does this "conjecture" rest on any other basis than the similarity of the effect produced by the sting of the nettle and the bite of the ant? I am inclined to think not. Certainly the fact that formic acid can be obtained from the nettle-plant is not in itself a cogent argument, seeing that it has

been shown that this acid is a widely-spread constituent of the cell-sap of living plants. The formic acid theory is also out of harmony with the fact that the fluid contained in the stinging-glands of the nettle has frequently, if not always, an alkaline reaction. It seems strange that we have at present no trustworthy observations on so interesting a question. Can none of our physiological chemists come forward and remove it from the region of conjecture?

ALFRED W. BENNETT

St. Thomas's Hospital, May 13

What is Histioderma?

CAN any of the readers of *NATURE* inform me to what class of fossil organisms belongs the genus *Histioderma*? Mention of the name—but of the name only—is made by Sterry Hunt in this journal, vol. vi. (1872), p. 54, and by Hicks in the *Quarterly Journal* of the Geological Society of London, vol. xxix. part 1, 1873, table facing p. 42. It does not occur in Broun's "Index Palæontologica," in Pfeiffer's "Nomenclator Botanicus," nor in Scudder's "Zoological Nomenclator." The name is not to be confounded with that of Carter's genus, *Histoderma*, established 1874, for recent sponges.

S.

Leyden, May 15

ON THE INFLUENCE OF FORESTS ON THE CLIMATE OF SWEDEN¹

A VALUABLE Report on this subject has been prepared by Dr. H. E. Hamberg, and printed as an appendix to the Report of the Forest Commissioners of Sweden for the year 1885. The observations were commenced in 1876, on the principles established by Dr. Ebermayer in Bavaria, but Dr. Hamberg soon found that the mere comparison of the results obtained at the forest station with those yielded by its sister station in the open country was insufficient to bring out all the peculiarities of forest influence, and accordingly he added a third class of station, situated in a clearing in the forest itself (*öppen plats i skogen*). The various results of these observations are discussed in a very exhaustive manner, and we must refer those interested in the subject to the Report itself. The author's conclusions, however, are very interesting, and are reproduced here in full.

"Our researches do not allow us to determine whether the presence of the forests on the whole contributes to increase or diminish the quantity of heat in the atmosphere, that is to say, to raise or lower its temperature. In fact, we have been entirely unable to take into account either solar radiation or the radiation from the needles² and the points of the trees. Until we are able to ascertain the quantity of heat which escapes from these surfaces, and its relation to that escaping from other surfaces, it is quite impossible to determine with certainty the influence of the forest on such an important subject as the mean temperature, and must confine ourselves to approximate estimations. Among the various surfaces which are met with in Sweden the most important are assuredly water, bare ground or rock, soil covered by herbage, and finally forest. Neither the surface of the lakes and sea nor the bare soil of town streets have any resemblance to the forest: the climate of the latter bears no similarity to a maritime climate or a town climate. A forest may best be considered as an instance of vegetation on a gigantic scale, as is evident from the low temperature of the ground under the trees, and the freshness of the air in summer, especially in the evening and at night-time, thus affording evidence of active radiation. In this case the forest would be a source of cold rather than of heat. But here we are simply dealing with suppositions.

"From this point of view a forest is distinguished from all the other surfaces we have mentioned, in that it extends into a stratum of air lying far above that in which man lives and carries on all of his occupations which depend on

¹ "Om skogarnes inflytande på Sveriges klimat." From *Quart. Journal Roy. Met. Soc.* for April 1886, communicated by Mr. R. H. Scott, F.R.S.

² The forests dealt with were entirely of pines and firs.